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the Behavior Therapist
Published by the Association for
Advancement of Behavior Therapy
305 Seventh Avenue - 16th Floor
New York, NY 10001-6008
(212) 647-1890/Fax: (212) 647-1865
www.aabt.org

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Subscription information: the Behavior
Therapist is published in 8 issues per year. It
is provided free to AABT members.
Nonmember subscriptions are available at
$38.00 per year (+$17.00 surface postage or
+$32.00 airmail postage outside USA).
Change of address: 6 to 8 weeks are required
for address changes. Send both old and new
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Steven C. Hayes, University of Nevada

There is something very disturbing about the recent article in IBT by
Corrigan (2001), arguing that ACT, FAP and DBT have gotten ahead of their
data. Staying in touch with the data is a defining feature of behavior therapy, and
it is no small matter to criticize behavior therapy scholarship on these grounds,
especially based on a proposed change in our historical methods of detecting this
problem. Losing touch with the data can precipitate extreme efficacy claims 10 years ago
e.g., that it is “by far the most effective and efficient treatment . . . “; Lipke,

2. The same criticism might apply to more recent behavior therapy innovations,
including ACT, and thus we need to compare “written claims about each therapy
to the set of published data” (Corrigan, 2001, p. 189, italics added).

3. Counting “supportive” nonempirical papers about ACT should suffice as “an
independent index that represents the claims made by the proponents”
(Corrigan, 2001, p. 189), which can then be compared to a list of Randomized
Controlled Trials (RCTs) located from PsycINFO as a measure of “the data
that exist to support these claims” (p. 189).

4. The author found 13 “supportive” nonempirical papers on ACT, as compared
to 10 empirical articles: 2 RCTs, 4 case studies, and 4 other empirical but less
well-controlled group studies, 2 of which were focused on process and 2 of
which were focused on outcome.

Corrigan’s Index

Corrigan’s argument was structured as follows (I will cast this in terms of ACT
because I cannot speak for the developers of FAP or DBT, but I suspect some sim-
ilar responses might apply there as well):

1. People have criticized EMDR
because it got ahead of its data when some
made extreme efficacy claims 10 years ago
(e.g., that it is “by far the most effective and efficient

2. People have criticized EMDR
because it got ahead of its data when some
made extreme efficacy claims 10 years ago
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3. Counting “supportive” nonempirical papers about ACT should suffice as “an
independent index that represents the claims made by the proponents”
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that exist to support these claims” (p. 189).

4. The author found 13 “supportive” nonempirical papers on ACT, as compared
to 10 empirical articles: 2 RCTs, 4 case studies, and 4 other empirical but less
well-controlled group studies, 2 of which were focused on process and 2 of
which were focused on outcome.
5. These numbers show that proponents of ACT are getting ahead of the data because “this ratio indicates the effort put into writing up claims about an intervention compared to writing up the data supporting these claims” (Corrigan, 2001, p. 191).

6. Therefore, proponents of ACT (and these interventions) have become “devotees of interventions that lack the data to support them” (Corrigan, 2001, p. 192).

7. This is a big worry because there is a risk that poorly supported treatments (presumably, such as ACT) will enter into practice guidelines.

This argument seemingly casts ACT, FAP, and DBT in a rather negative light, but the logic is poor. The proposed index does not comport with the behavior therapy tradition.

Detecting the Empirical Basis of Claims Requires Detecting Claims Themselves

A scientific claim is a particular kind of statement: one that reaches a scientific conclusion about relationships among events. Lipke’s statement above, that EMDR is “effective and efficient,” is a claim, for example. A bibliographic reference obtained by a computer search is not a “claim” until one knows what was said in that article. It is intellectually shallow and meaningless merely to count the number of theoretical articles as “an independent index that represents the claims made by the proponents” (Corrigan, 2001, p. 189). There is no such intellectual short cut. One must read the articles, examine the claims actually made, and assess the degree to which those claims fit with the evidence.

Corrigan explicitly rejects the idea that the amount of empirical work per se is even relevant to the issue of getting ahead of the data: “the point to consider here is not the raw number of published empirical papers . . . but rather the ratio of empirical to nonempirical papers” (2001, p. 191). I admire the consistency of the argument, since this stand is a logical requirement if his index is valuable, but this is itself a claim without empirical foundation. It seems mistaken. It would have us criticize writers about some of our most established scientific facts as “getting ahead of the data” if they write about the implications of these facts a bit too regularly. Facts with many implications are still facts.

According to Corrigan’s measure, the proportion of nonempirical articles is 48%, 57%, 77%, and 82% for EMDR, ACT, DBT, and FAP, respectively. Corrigan makes it clear that he considers these ratios to be too high, but he never provides a rationale for any particular ratio. To get a sense of how Corrigan’s metric would work in science more generally, I examined the results of similar searches I conducted on common scientific topics using the first 20 references located from the “Web of Science” databases maintained by the Institute of Scientific Information. The percentages of nonempirical articles were as follows for these topics:

- Theory of Relativity: 90%
- Space-Time: 85%
- Biological evolution: 75%
- Speed of light: 85%
- Heisenberg’s Uncertainty principle: 90%

By Corrigan’s measure, modern physics and biology are way ahead of their data.

The Perverse Effects of the Corrigan Index

Many factors could inflate Corrigan’s index. Thinking through a few of these shows how inadequate publication counts are as a method of discerning such a scientifically serious issue as the synchrony between claims and evidence. Here are some of the effects of his index.

Make Sure Your Work Is Not Considered Seminal

Seminal empirical articles in our field are sometimes cited frequently in texts, reviews, or theoretical articles. Seminal articles of this kind are thus necessarily making excessive claims, according to the Corrigan index. This is obviously false.

Make Sure Your Work Is Not Innovative

Truly innovative ideas are necessarily associated with excessive claims, according to the Corrigan index, both because of the discussion they engender, and precisely because they are new. Developers of new technologies have to explain their approaches and why they might be plausible, and try to show that they are something different than existing methods. Methods sections of empirical articles are entirely too brief to do this conceptually important work. Familiar methods do not require this same explication, and thus well-established methods in behavior therapy will usually have a low Corrigan index, while innovative methods usually will not. As a result, developers of any truly new approach will probably be condemned as “devotees” who are ahead of their data, even if they make only reasonable claims about the extant data, and they do not prematurely commercialize their work. The Corrigan index would...
have us strangle every new intellectual baby that is not in a mainstream crib.

Make Sure Your Work Is Not Philosophically or Theoretically Oriented

Some “dust bowl” empiricists think that data are all that matter, and that data speak for themselves. Those who view science differently will be penalized by the Corrigan index, especially if they value going through the difficult work of specifying and discussing assumptions and key concepts. The history of clinical science suggests that number crunching is not enough to build a discipline, but only that approach will make it through the numerical gauntlet of Corrigan’s index unscathed.

Make Sure Your Treatment Development Strategy Is Based Solely on a Relentless Series of RCTs

To me, the ultimate aim of behavior therapy is a progressive clinical science in which treatment, basic science, and applied theory form a coherent whole. If that is the aim, new technologies need to be philosophically coherent, based on a clear theory drawn from solid basic science evidence, and have evidence either that the process manipulated by the treatment is distinctive or that a known process is manipulated in some especially powerful way. The development of ACT embraced all of these steps, as is explained in some detail in the early chapters of the ACT book (Hayes, Strosahl, & Wilson, 1999). After a few outcome studies early in the development of ACT, we spent over a decade developing and testing the theory underlying the technology and examining the processes of change claimed to mediate the impact of the technology. We have recently published a book-length treatment of the theory that resulted, Relational Frame Theory (Hayes, Barnes-Holmes, & Roche, 2001). Relational Frame Theory is a comprehensive behavioral account of human language and cognition, based on over a decade of experimental work, and any serious critic of ACT must deal with this larger body of work. Only after that work was well established did we return to traditional RCTs (some of which are now appearing, e.g., Bach & Hayes, in press), and finally released the technology in book form 2 years ago. I do not and have not claimed that theoretical work substitutes for RCTs on ACT. To my mind, it supports and enables important RCTs on ACT. My point is that this method of treatment development, even though it is rigorous and experimentally based, will be called “getting ahead of the data” by Corrigan’s index because RCTs are not the sole focus.

Make Any Claim You Wish, as Long as It Is in an RCT

You can oversell your data shamefully and still be applauded by those who are attending to the Corrigan index. All you have to do is put your outrageous or overblown claims in the discussion section of an RCT. Voilà! You are not ahead of your data.

Rising to the Bait

Corrigan (2001) goes through various objections one might make to his analysis in defense of the treatment approaches he criticizes, such as the amount of basic science linked to the treatment, or the existence of other empirical articles that he may have missed. I do not propose to defend ACT in that way, because it takes the content of his criticism too seriously. I do not believe that his index can be the basis for worthwhile scholarly criticism, so no such defense is occasioned by the article. Still, I do want to state what Corrigan did not, namely, the claims we have actually made about ACT.

In the first book-length treatment of ACT, published only 2 years ago (Hayes et al., 1999), we reviewed all of the then-exist data and concluded, “We view the initial experimental evaluations of ACT as positive but preliminary” (p. 65). I stand behind that claim. If Corrigan thinks that this claim is “ahead of the data,” he should proceed in the normal, scholarly way: Examine the claim and show why it is false. Conversely, if, in accord with his index, he thinks there are too many conceptual articles about ACT, regardless of how conservatively they are crafted or of the actual intellectual value of the discussions they may contain, I can only wonder whether there are any behavior therapists who would want to turn over our field to the Vira Police. Respectfully, I do not.

There is one place where I have to rise to the bait. In order to make good his assertion that we are ahead of the data on ACT, but without having to examine what we actually say about it, Corrigan tries to link ACT to a rebellion against the scientific method. Without referring to any specific pages or quotes, he cites the entire ACT book (Hayes et al., 1999) as a source for his statement that some authors have “described the limitations of RCTs and the need for additional ways of defining what is evidence” (Corrigan, 2001, p. 192). He then begins the very next sentence, “Although they have posed some eloquent arguments against the scientific method . . . “ There is no basis in the ACT book, or indeed in anything I have written, for such a statement. Readers of this publication are likely to assume that there isn’t, since I have been fortunate to serve recently as president of AABT and have in that role regularly written about the importance of science to clinical work.

New behavior therapy technologies should certainly be held to the highest scientific standards. These standards include RCTs (or an extensive series of case experimental designs). We make no arguments against RCTs in the ACT book, and indeed I mention some of the RCTs we have under way there. We were criticized by Corrigan for doing that as well, but we mentioned them not to make specific empirical claims but precisely in order to emphasize that we were committed to holding ACT to high standards of evidence. I do think that empirical clinical science needs more than RCTs, along the lines argued above, but a concern for basic processes and theory is certainly not an argument “against the scientific method.” Perhaps Corrigan had some other sections of the book in mind, but I cannot think what they might have been.

The Future of ACT Research

It has only been 2 years since ACT has been described in full technical detail. The empirical literature on ACT has grown rapidly since (it is about twice as extensive as Corrigan describes, particularly because of a rapid increase in published case studies around the world). Several well-controlled and sizeable RCTs are indeed in press, under review, completed, or are on their way. I do not make this latter statement to increase the empirical confidence in ACT but by way of saying that ACT has been and will continue to be developed in a scientifically conservative way. The effort we have put into the development and experimental testing of the contextual theory of cognition that underlies ACT goes beyond similar efforts by developers of any contemporary psychotherapy of which I am aware. Still, as of this moment, my personal conclusion about the empirical support for ACT has not changed: The data are positive but preliminary. In a few years we will know much more about whether ACT is useful, and how it works, as these and other studies move through peer review.

Even if all of this empirical work is supportive, however, ACT will never look scientifically conservative by the Corrigan index. ACT is counterintuitive, philosophically rich, and theoretically interesting. It has important implications for understanding human functioning more generally, if the theory underlying it is sound. If additional positive data come in, ACT and Relational Frame Theory will be more believable, and the discussions and extensions will only increase, not decrease. Our Corrigan index will worsen. Apparently, some members of the Vira Police will not
be pleased, but to my eyes, this is a problem devoutly to be wished.

References


Letter to the Editor

Getting Ahead of the Data: Not All Threats Are Equal

Scott T. Gaynor, Western Michigan University

Behavior therapy as a movement is unique in tying intervention efforts to basic psychological science and evaluating intervention outcomes empirically. Functional Analytic Psychotherapy (FAP), Acceptance and Commitment Therapy (ACT), and Dialectical Behavior Therapy (DBT) emerge from this tradition (Nelson-Gray, Gaynor, & Korotitsch, 1998), but are they getting ahead of the data? This possibility served as the foundation for a recent paper by Corrigan (2001).

A therapy can be said to be getting ahead of the data when dissemination efforts and efficacy/effectiveness claims go beyond what might be reasonably concluded from the available clinical outcome findings. I will comment on these two areas with specific emphasis as to how they apply to FAP, the approach given the least coverage by Corrigan.

Claims of Efficacy/Effectiveness

Corrigan (2001) reports that 82% (14/17) of the published papers on FAP are supportive descriptions, while there are no studies of clinical outcome (neither randomized clinical trials nor single-subject designs). The implication from this index (descriptive publications divided by empirical publications) is that FAP is guilty of getting ahead of the data. Corrigan’s summary is accurate in its facts and FAP has had a rather long incubation period between the first systematic descriptions (Kohlenberg & Tsai, 1987) and the emergence of any empirical data. However, theoretical descriptions speaking to the plausibility of efficacy are not tantamount to strong claims of current efficacy. Indeed, there is no mention by Corrigan of specific claims made by proponents of FAP that are deemed excessive or extraordinary. Thus, if the claim is that FAP needs more empirical support, then FAP is guilty as charged, and Corrigan should find wide agreement on this point. However, there is no evidence presented that FAP is guilty of the stronger charge of making excessive, premature claims of efficacy.

But what are some of the claims made regarding FAP? FAP was not created to treat any particular disorder and, to my knowledge, no claim is made that FAP is the treatment of choice for any currently recognized DSM disorder. In fact, published descriptions of FAP (see Kohlenberg & Tsai, 1994) and conference presentations on FAP (Kohlenberg, Bolling, & Parker, 1997) often present it as a treatment that can be used to augment an empirically supported treatment (e.g., CBT for depression), if, for instance, CBT is not producing a completely satisfactory response. In other words, FAP, it appears, is currently recognized as an experimental approach that may be useful in augmenting other established treatments or as a treatment of second resort when others have failed.

Dissemination

Before going on, I should note that as a graduate student I took a seminar on FAP and since then I have attended workshops and presentations on FAP at AABT and ABA. Thus, I might be considered a “devotee” or, worse, a victim of the exces-
sive dissemination of FAP. However, dissemination serves multiple functions, at least two of which Corrigan lumps together: dissemination to the public (e.g., marketing of the treatment) versus dissemination to the professional/research community. The former emphasizes adoption of the treatment, the latter serves the additional function of informing an audience trained to conduct efficacy and effectiveness research of a treatment in need of study. FAP should not be marketed to the public as best practice, but theoretical/descriptive publications in professional journals and conference workshops appear to be more of the latter ilk. Furthermore, current standards for evaluating efficacy point to the importance of treatment effects being demonstrated by at least two different teams of investigators (Chambless & Ollendick, 2001). Such replication across investigators requires other researchers to be familiar with the theory and practice of an intervention prior to it being deemed well-established. How should this familiarity be developed? Publication of comprehensive texts and presentations at professional conferences appear appropriate means.

A Modest Example

Like Corrigan, when first introduced to FAP, I too was struck by the absence of an empirical database. As one trained in the scientist-practitioner model, the lack of data served as an important motivating condition for beginning to investigate this type of treatment. My interest was in applying an FAP-like approach with depressed adolescents. The following paragraphs briefly review some preliminary work Scott Lawrence and I did in this area. The data reviewed do not bear directly on the question of efficacy, but provide some initial information as to the feasibility of combining an FAP-like approach with CBT and whether there is evidence of an iatrogenic effect—important early questions in treatment development. More importantly, this work illustrates how exposure to an experimental treatment approach need not foster blind devotion, but rather may encourage relevant empirical investigations.

The first step was to attempt to apply FAP along with CBT in the treatment of an individual case. The client was a 17-year-old female seen approximately 3 weeks postdischarge from an inpatient hospitalization for major depressive disorder. The time series BDI data are presented in Figure 1 and were suggestive of a clinically significant change during the time in which she was in treatment.

Following successful treatment of this single case, a group treatment based on FAP (called Learning Through In-Vivo Experience: LIVE) was developed and then delivered in conjunction with the Adolescent Coping With Depression course (CWD-A). CWD-A is a CBT that has outperformed a wait-list comparison condition in two randomized clinical trials (see Clarke, Rohde, Lewinsohn, Hops, & Seeley, 1999; Lewinsohn, Clarke, Hops, & Andrews, 1990). CWD-A + LIVE was conducted with two groups, each consisting of 5 teens with significant symptoms of depression, using a nonrandomized, single-subject approach (see Gaynor & Lawrence, in press). The adolescents who completed the treatment (n = 8) improved from pretreatment to posttreatment, and those who discontinued treatment (n = 2) did not. Among the 8 treatment completers, 6 showed evidence of clinically significant change. Moreover, the mean change in BDI units for these 8 participants was 13.5 (SD = 3.9), similar to that reported in randomized clinical trials of CWD-A (range: 11.7 to 16.4). Finally, the treatment produced strong group cohesion, adolescents and their guardians rated the intervention positively, and improvements were maintained at 3-month follow-up (Gaynor & Lawrence).

These data provide some preliminary evidence that it is feasible to complement CWD-A with an FAP-based approach, and that doing so does not appear to have harmful effects. The results do not speak directly to the efficacy of the FAP-based approach, as there was no way to disentangle the effects of CWD-A from those attributable to LIVE. However, the approach of moving from careful use with single cases to an open clinical trial format is consistent with the model adopted by the National Institutes of Health for development of psychotherapeutic treatments. The reviewed work is consistent with the goals of the earliest stage: “develop the treatment and pilot test it with a basic design” (Najavits, 1998, p. 177).

Conclusion

Corrigan’s (2001) article addresses an important issue in clinical science—going beyond the data in promoting and disseminating new treatments—and one that should not be taken lightly. We must not buy into FAP, ACT, DBT, or any other treatment without appropriate skepticism and solid empirical findings. Such empirical findings do not yet exist for FAP. However, in and of themselves, published descriptive/theoretical articles are not tantamount to making strong claims about efficacy, nor are giving workshops and conference presentations equivalent to marketing a treatment as best practice to the public or policymakers. In fact, dissemination to professional colleagues appears to be one useful way to stimulate research, a modest example of which was offered.

References


Figure 1. Time-series BDI scores across pretreatment (Pre), weekly acute treatment, and maintenance follow-up treatment after 2 months away from treatment. Note: R = Retrospective BDI estimating symptoms at 2 weeks prior to treatment initiation. I = Intake BDI capturing functioning in week prior to treatment.


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**The deadline for submissions is October 15, 2002.**
Letters to the Editor

The Data Is Still the Thing: A Reply to Gaynor and Hayes

Patrick Corrigan, University of Chicago

I have different replies to the two responses to my recent paper (Corrigan, 2001). Thanks to Gaynor (2002) for providing single-subject evidence about Functional Analytic Psychotherapy (FAP). My goal was not to dismiss FAP or any of the other therapies as ineffective. Rather, I wanted to encourage researchers to look at the data, much as Gaynor has done here. It also seems that Gaynor and I agree that “going beyond the data in promoting and disseminating new treatments” should be a matter of concern to behavior therapists. We seem to part company in considering when specific therapies fall in this error. Gaynor seems to view behavior therapy more liberally, suggesting the dissemination of FAP before its empirical findings are obtained serves the purpose of promoting discussion and research. I have a more conservative view. Therapies and data are co-synchronous; one should not precede the other.

I have two responses to Hayes (2002). First is whether there was any utility in my literature analysis. The original paper acknowledged that this kind of meta-analysis has its limitations, as methodologists have discussed elsewhere (Wolf, 1986). Despite these limits, the paper conservatively pointed out some of the reasonable implications for which behavior therapists need to be vigilant. Hayes focused on its limitations by noting that a review of 20 recent references generated by a library database would yield unreasonable assumptions about such important scientific concepts as relativity and biological evolution. According to his analysis, less than a quarter of the articles are empirical and so, applying my rationale, these ideas are ahead of their data. But there is a major difference in Hayes’s review and my effort to track where four behavior therapies fell in the literature. Hayes examined only 20 papers about these topics that generate thousands of hits in reference databases. I reviewed all the papers generated by the dominant databases in mental health services. Hayes’s argument should not obscure what is intensely sobering about the simple findings of my earlier analysis. Behavior therapies as prominent as DBT and ACT rest on empirical studies where 56 and 11 patients, respectively, received the treatment.

Of more concern is Hayes’s view that my critique represents “vita” assault. Somehow the argument turned away from the strengths and weaknesses of individual therapeutic systems to perceived attacks on the researchers who developed them. This is by no means my goal and, in fact, would be a terrible regression for behavior therapy. Prior to the rise of behavioral interventions, psychotherapy was dominated by the systems and theories of charismatic figures and their devotees. We had Freudian, Jungian, Adlerian, Rogerian, Ericksonian, Perlsian, and other therapeutic systems from which practitioners might choose based on its wisdom and rationale. With the introduction of behavior therapy, the nature of evidence changed from authoritarian epistemologies to empirical method, from clinical wisdom to observable data. This metamorphosis is clearly evident within our discipline. We do not participate in Wolpean therapy, but, instead, discuss systematic desensitization. We are not Beckians but cognitive therapists. And when critics question the assumptions of systematic desensitization or cognitive therapy, they are not attacking Wolpe and Beck. They are challenging the evidence-based theory that these two researchers had a part in creating.

Behavior therapists need to continue to be vigilant about whether individual therapies get ahead of the data, whether dissemination of a treatment is prominently displayed prior to the completion of careful research.

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The Dissemination of Novel Psychotherapies: A Commentary on Corrigan, Gaynor, and Hayes

James D. Herbert, Drexel University

When it comes to the evaluation of the effectiveness and the dissemination of psychotherapy, things often turn out to be far more complicated than they first appear. So it is with the provocative essay by Corrigan (2001). At first glance, Corrigan seems to make a point so obvious that all behavior therapists would readily embrace it: Our assessment and intervention methods should be tied as much as possible to science, and dissemination of these technologies should be kept in step with empirical developments. It’s hard to argue with these general principles. But like so many general principles, the devil is in the details. For example, what kind of science should underlie our technologies? How much research is enough before dissemination? And on what basis do we judge the appropriateness of specific dissemination efforts?

Corrigan’s solution to these issues centers on an index composed of the number of empirical studies of an intervention in relation to the number of nonempirical publications in which the intervention is described or discussed in the literature. Papers of the former type are held to represent the scientific support of a therapy, whereas those of the latter type are thought to represent its dissemination. The idea is that the lower the index, the less empirical support an intervention has relative to the degree of effort put into its dissemination. Using this metric, Corrigan compares four innovative therapies that have been featured in several AABT programs over the past decade: Eye Movement Desensitization and Reprocessing (EMDR; Shapiro, 1995), Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, & Wilson, 1999), Dialectical Behavior Therapy (DBT; Linehan, 1993), and Functional Analytic Psychotherapy (FAP; Kohlenberg & Tsai, 1991). He notes that EMDR has been widely criticized for “getting ahead of the
data,” but observes that its score on his proposed index is higher (indicating a higher percentage of empirical to nonempirical papers) relative to the other three therapies, which have not been subject to the criticism and heated controversy surrounding EMDR. On this basis, Corrigan suggests that the dissemination of novel therapies such as ACT, DBT, and FAP has proceeded beyond what is justified by the empirical literature.

Problems With Corrigan’s Index

Both Gaynor (2002), speaking from the perspective of FAP, and Hayes (2002), as the founder of ACT, raise serious questions about the utility of Corrigan’s index. Most fundamentally, both Gaynor and Hayes argue that the number of descriptive papers on a technique cannot serve as a proxy for the specific claims made in those papers. In order to assess the “evidential warrant” (McNally, in press) of a scientific claim, one must begin by directly examining the claim itself. It is not enough to count the number of papers that have been published on a topic, since this reveals nothing whatsoever about the nature of the claims made about it. Thus, Corrigan’s index is uninformative with respect to the evidential warrant of any specific claims made on behalf of a therapy.

Gaynor (2002) makes an important distinction between dissemination to the professional (and especially research) community and dissemination directly to the public. He notes that the former is a necessary condition for independent research on a treatment, whereas the latter should be approached with greater caution until a sufficient research base has accumulated. Corrigan’s (2002) discussion of dissemination misses this critical distinction. In fact, as elaborated below, the distinction between these two types of dissemination is one of the reasons EMDR has been criticized so much more than ACT, DBT, or FAP.

Hayes (2002) discusses several disturbing implications of Corrigan’s index. He notes that therapies that are highly innovative, philosophically or theoretically oriented, and/or seminal are likely to be cited frequently, which will deflate the empirical index. To maintain a high index of empirical to nonempirical papers, it follows that one should do lots of treatment outcome research, but should not write about it too much. Moreover, one should hope that others likewise refrain from writing about it. Hayes points out the absurdity of this position by illustrating that the literature on several well-established scientific topics (e.g., the theory of relativity, the speed of light) is composed of much higher percentages of nonempirical articles than is the literature on any of the psychotherapies discussed by Corrigan. This is a natural consequence of a highly influential scientific theory, principle, or application.

Hayes (2002) also criticizes Corrigan’s (2002) exclusive reliance on randomized clinical trials (RCTs) as the index of empirical support for an intervention. Hayes obviously places greater importance on the basic scientific research underlying a technology and the link between that research and its applications than does Corrigan, who emphasizes that the question of a treatment’s efficacy and the validity of the theory underlying it are distinct issues. Reasonable scholars can disagree on this point. Hayes notes that one of the unique features in the history of behavior therapy is the link between basic and applied research, and that this link is completely ignored by Corrigan’s index. Corrigan writes, “it is an error in logic to assume that the body of data underlying an intervention supports efficacy claims of that intervention” (p. 191). Although this difference in emphasis is well within the bounds of scholarly debate, Corrigan carries his admiration of RCTs too far when he suggests that the proponents of ACT and FAP (among others) “have posed some eloquent arguments against the scientific method” (p. 192). There are two serious problems with this statement. First, it naïvely equates RCTs with “science.” Second, it suggests that the proponents of DBT, ACT, and FAP flatly reject RCTs. Both Gaynor and Hayes clearly disavow this characterization on behalf of FAP and ACT, and my understanding is that the same could be said of the developer of DBT. An appreciation of the important role of basic theoretical research, well-controlled single-case designs, and even certain quasi-experimental designs does not diminish the critical importance of RCTs. Despite their importance, one problem with relying exclusively on RCTs to identify empirically supported treatments is that a new RCT would be required for every minor technical innovation, thereby slowing down the field enormously. Along with certain critical RCTs, grounding treatments in basic and analogue research is likely to lead to more efficient development of empirically supported technologies than an endless series of RCTs.

Corrigan’s Reply

Corrigan (2002) offers distinct replies to Gaynor and to Hayes. To Gaynor, he reasserts his view that dissemination of novel therapies such as FAP should await further data. But he does not clarify how much data would be enough, nor does he...
address Gaynor’s important distinction between different kinds of dissemination.

Corrigan takes issue with Hayes’s review of established scientific topics on the grounds that Hayes examined only a representative sample of the literature on these topics, whereas Corrigan reviewed all of the available papers in his review and in the calculation of his indices. It is not at all clear, however, why this should matter. Corrigan’s implication is that had Hayes examined every available paper on, say, biological evolution (a truly Herculean task!), a far greater number of empirical papers relative to descriptive papers would have been located, and the index would have been quite different. This conclusion is inconsistent with basic sampling theory, and Corrigan offers no explanation for why an exhaustive literature search would make a difference. Although it might be argued that Hayes’s sample was not truly random, there is no reason to expect that it was biased in favor of nonempirical publications. Corrigan’s reply does not address Hayes’s other concerns with his proposed index.

Corrigan does make it clear that his concerns about the dissemination of novel therapies should not be construed as ad hominem attacks on the developers of these therapies. “Somehow the argument turned away from the strengths and weaknesses of individual therapeutic systems to perceived attacks on the researchers who developed them” (p. 140). Interestingly, I did not perceive ad hominem arguments in either Corrigan’s original essay (2001), or in Gaynor’s (2002) or Hayes’s (2002) responses. Although the debate was spirited, it did not strike me as personal. I suspect that Corrigan’s perception might have resulted from the title of Hayes’s essay (“On Being Visited by the Vite Police”), which might be taken to suggest a personal assault on the work of an individual researcher.

Finally, in his reply Corrigan (2002) seems to move away from his proposed index in favor of the absolute number of subjects treated:

Hayes’s argument should not obscure what is intensely sobering about the simple findings of my earlier analysis. Behavior therapies as prominent as DBT and ACT rest on empirical studies where 56 and 11 patients, respectively, received the treatment. (p. 140)

Setting aside, for the sake of argument, the accuracy of these numbers, this raises a different issue. In the original essay, Corrigan (2001) was clear that his concern was with the ratio of empirical to descriptive papers, rather than the raw number of empirical studies, and it was this ratio that both Gaynor and Hayes criticized. In his reply, Corrigan (2002) moves away from the ratio in favor of the absolute number of participants treated in controlled studies. As discussed above, one problem with this approach is Corrigan’s equating “empirical studies” exclusively with RCTs—an idea foreign to behavior therapy. Moreover, Corrigan offers no guidelines as to how many participants should be treated, and how successful that treatment should be, before dissemination takes place.

Other Problems With Counting Studies

There are two additional related problems with Corrigan’s proposed metric that neither Gaynor nor Hayes discussed directly. The number of empirical studies supporting a treatment reveals nothing about the methodological strength of those studies, nor the results obtained. A hundred studies of a new intervention may appear impressive, but if they are all seriously flawed they will be much less informative than a handful of well-controlled studies. Moreover, simply counting studies is not informative with respect to the direction or magnitude of the results. Issues of effect sizes, clinical significance, and external validity are ignored. Any serious review of a body of empirical research must address these issues.

So What About EMDR?

By contrasting ACT, DBT, and FAP on the one hand with EMDR on the other, one senses a slightly subversive theme to Corrigan’s paper. ACT, DBT, and FAP have not been especially controversial to date, whereas EMDR has attracted widespread criticism within the scientific community; and yet, according to Corrigan’s index, EMDR seems to fare better. Although Corrigan states that he is not rendering judgments on the efficacy of any individual therapy, the clear implication is that many of the concerns that have been raised about EMDR should be applied equally, or even more forcefully, to ACT, DBT, and FAP. So why has EMDR borne the brunt of scientific criticism? Are the critics somehow biased against EMDR relative to other innovative therapies?

A critique of the scientific status of EMDR is beyond the scope of this essay; the interested reader is referred to recent reviews by Cahill, Carrigan, and Frueh (1999), Davidson and Parker (2001), Herbert et al. (2000), Lohr, Tolin, and Lilienfeld (1998), McNally (1999), and Rosen, Lohr, McNally, and Herbert (1998). Nevertheless, contrasting EMDR with the other novel therapies Corrigan discussed illustrates the limitations of his proposed index as a grounds for judging dissemination efforts, and also clarifies why EMDR has been targeted for criticism more than ACT, DBT, or FAP. EMDR differs from the other three therapies in many ways. Of specific relevance to Corrigan’s analysis are four issues: (a) the nature of the specific claims made about each therapy, (b) the way in which proponents of each therapy deal with unsupportive data, (c) the scientific status of the theories underlying the therapies, and (d) the specific nature of dissemination efforts.

Claims

The developers of ACT, DBT, and FAP have been relatively modest and conservative in their claims regarding their respective therapies. The quotations offered by both Gaynor (2002) and Hayes (2002) from the texts on FAP and ACT, respectively, illustrate this reserve, and Hayes explicitly describes the empirical status of ACT as “positive but preliminary.” This conservative tone has been clearly reflected in statements made at symposia and workshops on ACT, DBT, and FAP at AABT meetings. In contrast, the claims made about EMDR are truly mind-boggling. Promotional brochures published by the EMDR Institute, Inc., feature testimonials describing EMDR as uniquely efficient and effective for trauma-related problems. Workshops are offered describing the application of EMDR to a bewildering array of issues, including traumatic memories, depression, smoking cessation, somatic disorders, menopause, sexual harassment, and “inner child” syndromes, among others. Francine Shapiro, the developer of EMDR, describes the technique as a “paradigm shift” that integrates all of the major schools of psychotherapy (1995). It has been described in the popular press as a “miracle cure” (Stone, 1994).

Unsupportive Data

Science advances through criticism. When data consistently fail to support one’s hypotheses, the appropriate course of action is to modify or eventually abandon the theoretical position from which the hypotheses were derived. The limited research on ACT, DBT, and FAP has not yet yielded data that directly contradict important hypotheses associated with each approach. It is therefore impossible to conclude at this time how their proponents would respond in the face of contradictory data. Nevertheless, it is important to note that each of these therapies is associated with potentially falsifiable hypotheses. For example, Hayes and colleagues (1999) hypothesize that ACT tends to impact the believability of dis-
tressing thoughts, primarily, and their frequency, secondarily, whereas traditional cognitive therapy typically shows the opposite pattern. This hypothesis is testable. If data consistently failed to support it, we would have to see how Hayes responded. In the case of EMDR, data have in fact consistently failed to support central tenets. For example, a body of research has shown that EMDR is no more effective than other established therapies for trauma-related conditions and other anxiety disorders (and may even be less effective, especially over the long term), and that the defining feature of the intervention—eye movements—does not add to the effectiveness of the technique. Yet the proponents of EMDR cling to precisely these hypotheses, in spite of the data (Lohr, Lilienfeld, Tolin, & Herbert, 1999; Rosen, 1999).

Theory

Related to the issue of the role of criticism in advancing science is the scientific status of the theory underlying a psychotherapy. ACT, FAP, and DBT are all based on ideas that are plausible, testable, and consistent with a body of basic psychological research. ACT in particular is closely tied to a body of basic theoretical work that encompasses scores of experimental studies (Hayes, Barnes-Holmes, & Roche, 2001). EMDR, in contrast, is based on a theory that is implausible, overdetermined to the point of being difficult if not impossible to falsify, and poorly linked with basic psychological science (Herbert et al., 2000; McNally, 1996; O'Donohue & Thorp, 1996).

Dissemination

Finally, as Gaynor discussed, not all dissemination efforts are alike. Dissemination of an intervention directly to the public, particularly if associated with claims of unique efficacy, should require a high threshold of evidence. In contrast, dissemination to a professional scientific audience is a prerequisite to the generation of just such a research base. (It should be acknowledged, of course, that not all audiences fit neatly into these two categories.) The books and workshops that have been written on ACT, DBT, and FAP are clearly intended for professional audiences. Although proponents of EMDR have written a few scholarly texts (e.g., Lipke, 2000; Shapiro, 1995), far more books have been written directly targeting the general public, with titles such as EMDR: The Breakthrough Therapy for Overcoming Anxiety, Stress, and Trauma (Shapiro & Forrest, 2001) and Emotional Healing at Warp Speed: The Power of EMDR (Grand, 2001). In addition, Web sites designed for the public extol the many virtues of EMDR (e.g., www.emdr.com; www.emdria.org; www.emdr-europe.net).

The point of this discussion is that EMDR differs systematically from the other innovative therapies discussed by Corrigan (2001) in several ways that are not captured by his index. Moreover, examination of these differences reveals the source of some of the criticisms of EMDR, and the reasons why ACT, DBT, and FAP have (so far, at least) escaped such criticism. Corrigan’s use of EMDR as a foil against which to make a point about the dissemination of novel therapies does not address the many ways in which EMDR differs from the other innovative therapies he discusses.

Conclusion

Corrigan raises a very important (and seemingly uncontroversial) point about not letting dissemination efforts outstrip relevant data. When the discussion becomes more specific, however, things quickly become messy. The metric he proposes to reflect the appropriateness of dissemination efforts relative to the empirical research is highly misleading. The index ignores many critical issues that must be considered in evaluating the dissemination of novel therapies, including (a) the specific claims made about a treatment, (b) the methodological status of the studies that comprise the literature, (c) the actual nature of the results of those studies, (d) the audience to whom the dissemination efforts are directed, and (e) the status of empirical and theoretical work that supports the therapy other than RCTs. Furthermore, Corrigan’s use of EMDR as a standard against which to compare other novel therapies ignores the basis of much of the controversy surrounding EMDR. The appropriateness of various dissemination efforts in relation to the empirical status of novel therapies is an important topic, and Corrigan should be commended for raising it. The issues are complicated, however, and defy simple formulæ. Perhaps we can all agree on at least two general points. First, we need more data on innovative behavior therapies. And second, the proponents of new therapies should exercise caution in keeping their specific claims consistent with the empirical literature. Operationalizing these apparently obvious principles will not be as straightforward as it might appear, and will require further healthy discussion and debate.


Open Forum

TREATMENT OUTCOME ASSESSMENT PRACTICES OF PSYCHOLOGY TRAINING CLINICS

John D. Tyler, Michael A. Busseri, and Alan R. King, University of North Dakota

Provider accountability and the empirical validation of treatment procedures are major trends in contemporary mental health care that encourage efforts to assess psychotherapy outcome. However, in the context of training the next generation of service providers, such interests must compete with other concerns. For example, does the prospect of treatment outcome measurement distract neophyte clinicians from client issues or contribute to excessively elevated anxiety levels?

Whether to routinely conduct formal assessments of psychology training clinic treatment outcomes is a question with a lengthy history. The majority of articles that address the question (e.g., Halgin, 1986; Todd, Jacobus, & Boland, 1992) have strongly advocated for the practice. Serifica and Harway (1980) note that support for assessment of training clinic treatment outcomes can be found as early as 1896 in a proposal to the American Psychological Association (APA) by Lightner Witmer, the founder of the first psychological clinic. Advocates for the practice have argued that routine collection of formal treatment outcome data represents a core value of the scientist-practitioner model (Halgin; Todd et al.). Others have similarly noted that routine assessment of clinical outcomes nicely models the integration of research and practice (Messer & Boals, 1981; Serifica & Harway). The potential benefits of formal treatment outcome assessments (FTOAs) in providing both heuristic feedback to trainees and supervisors and quality control of training clinic enterprises could also be cited. Recent efforts at Penn State University (Borkovec, 2001) to develop a national practice research network (PRN) include efforts toward an outcome data-base from university training clinics. Members of the Counsel of University

On these Web sites you will find material about the diplomate (board certification) in behavioral psychology, benefits of holding the credential, application forms, behavioral psychology archival information, a directory of diplomates, news items, and organizational information. I invite you to visit these Web sites and to contact me if you have any questions.

—E. THOMAS DOWD, PH.D., ABPP, PRESIDENT

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Directors of Clinical Psychology (CUDCP) training programs discussed the Borkovec initiative at their January 2001 meeting and PRN panel discussions are planned for upcoming APA and APS conventions.

Despite the history of support for conducting FTOAs, past reports have indicated that the proportion of psychology training clinics that routinely conduct outcome assessments has been low to moderate, ranging from 27% to 68% (Messer & Boals, 1981; Stevenson & Norcross, 1985). In addition, a variety of problems associated with conducting such evaluations have been identified. Halgin (1986) reported that 55% of training clinic directors identified conducting such evaluations as problematic, citing logistic difficulties, followed by “priorities” (45%) and lack of faculty interest (44%). Halgin also reported that clinic trainees expressed concern that outcome research might intrusively impact therapy as well as add to their anxieties as beginning therapists. Also relevant may be Moore and Kenning’s (1996) observation that training clinics are typically less dependent on funding sources that require such accountability.

In the present investigation we sought to determine the percentage of psychology training clinics that currently conduct routine outcome assessments and to inquire about both benefits and disadvantages of such evaluations. In addition, we asked about several practical issues associated with evaluating the treatment efforts of those still in training: What sorts of assessment instruments are typically used in evaluating outcome? Is client outcome data used solely to guide trainee mentoring or is it also used to evaluate the trainee’s supervisor? What has access to the data?

Method

Treatment Outcome Assessment Surveys were sent to the psychology training clinic directors at the 193 accredited clinical psychology programs listed on the APA Web site as of March 31, 2000. Approximately 2 months later, a second copy of the survey was sent to the same programs, along with a letter asking recipients to respond to this mailing if they had not returned the first questionnaire.

The survey questionnaire contained 12 items, several of which included subparts. The first item asked if the respondent’s clinic “routinely conducts formal treatment outcome assessments (FTOAs).” “Formal assessments” were defined as “those in which there are multiple administrations of psychological tests, target symptom rating scales, or similar instru-

ments.” Those who responded no to this question were asked to indicate whether some individual supervisors conducted FTOAs at their facility, and, if so, the proportion who did. They were also asked to identify reasons that supervisors did not use FTOAs.

Respondents who reported that at least some supervisors at their clinics used FTOAs were then asked to respond to questions about how such information was collected and used and about perceived benefits and problems associated with the collection of such assessment data.

Results

Of the 193 surveys mailed, 77 replies were received, yielding a return rate of 40%. Six of these (3% of all surveys mailed) indicated that the program had no training clinic. Consequently, there were 71 usable returns. Of the 71 respondents, 56% (n = 40) reported that FTOAs were routinely conducted at their clinics. On the remaining 31 returns, 15% (n = 11) reported that some clinic supervisors conduct treatment outcome assessments and 28% (n = 20) indicated that FTOAs were not routinely conducted. Ten of the 11 clinics at which only some supervisors conduct such assessments supplied enough information to permit calculation of the percentage of supervisors who did. For these settings, on average, 37% (ranging from 4% to 83%, n = 10) of the supervisors conducted FTOAs.

Responses from the 40 clinics at which all supervisors conduct FTOAs indicated that at 90% (n = 36) of these sites, cases from all student therapists were included in the FTOA database.

Respondents from the 20 clinics at which FTOAs were not routinely conducted were provided an open-ended question to identify reasons why this practice had not been adopted. The most frequently cited reason (cited by 53%, n = 10) involved lack of consensus among supervisors (e.g., disagreement about the importance of measurement or about which measures were appropriate). The next most frequent reason (37%, n = 7) involved limited resources (e.g., limited time, high dropout rate for clients). Concerns about lack of usefulness were voiced by 26% (n = 5). Plans to develop a FTOA program were mentioned by 26% (n = 5).

Respondents from the 51 clinics at which at least some supervisors conduct routine formal treatment outcome assessments answered several questions about their experiences with this practice as it relates to their clinic as a whole. Their responses are presented next. Because the number of clinics that replied to each question varied, that number is presented for each item reported.

Data Uses

When asked to indicate for which of several options FTOA data were used, 84% (n = 42 out of 50) endorsed “training purposes,” 84% (n = 42) endorsed “quality monitoring/quality control,” and 59% (n = 30) indicated use of this data for research purposes. Interestingly, 30% of this latter group (n = 9 out of 30) indicated that FTOA data-collection procedures had not been approved by Institutional Review Boards (IRBs) as did 58% (n = 28 out of 48) of the entire subsample of respondents who replied to a question about IRB approval.

Respondents were also asked whether in their settings FTOA data were used for trainee or staff evaluation purposes. Use in routine student therapist evaluations was relatively rare, with only 15% (n = 7 out of 49 respondents) reporting this practice. Even less prevalent was use of outcome data to evaluate clinic supervisors, with only one setting (n = 50 respondents) acknowledging this practice. None of 28 respondents indicated that such data were used in making promotion, tenure, or salary decisions.

Access to Data

Clinic directors were next provided a generic list of clinic and training staff and asked to indicate for each whether access was allowed to FTOA data, and, if so, whether access was restricted (“permission must be given first”) or open (“no permission needed”). Results indicated that open access was given to the supervisor of the case at 35 out of 44 respondent sites (80%), the student therapist for the case at 34 sites (77%), and the clinic director at 36 sites (82%). Less common was the practice of affording restricted access to case supervisor (reported by 18%, n = 8), therapist (23%, n = 10) and director (11%, n = 5).

Access to FTOA data for others was generally less common. Open access was relatively rare for other clinic supervisors (n = 4, 9%) and other student therapists (n = 4, 1%) and for nonclinical faculty (n = 0). Restricted access for other clinic supervisors (n = 1, 2%), other therapists (n = 18, 42%), and nonclinical faculty (n = 10, 23%) was a more common practice.

Benefits of Conducting FTOAs

Next, respondents were asked to rate the following on a 5-point scale: “How helpful has the FTOA data been to your clinic?” (1 = not at all helpful, 5 = very helpful).
bhelpful). A mean rating of 3.51 (SD = 0.97, n = 45) was obtained, with 51% of respondents (n = 23) endorsing either 4 or 5 on the scale. Seven respondents volunteered that they had not yet collected a great deal of FTOA data (e.g., “We’ve just started to collect data,” “Our N is too small”). With their ratings removed, results for the remaining respondents yielded a mean helpfulness rating of 3.71 (SD = 0.84, n = 38).

Survey recipients were also given an open-ended question asking for a listing of “benefits of conducting FTOA at your clinic.” Forty-one responded, many citing more than one benefit. Training of students (n = 18, 44%), treatment aids such as assessment and planning (n = 17, 41%), and quality control (n = 12, 29%) were most frequently mentioned, with research mentioned by 17% (n = 7).

Problems With FTOAs

Clinic directors were then asked to rate on a 5-point scale, “How problematic has the collection of FTOA data been to your clinic?” (1 = no problem at all, 5 = very problematic). A mean rating of 2.94 (SD = 1.07, n = 47) was obtained. The average problem rating for clinics in which all supervisors use FTOA data was not significantly different than that for clinics in which only some supervisors collect FTOA data, Ms = 2.88 vs. 3.29, SDs = 1.02 vs. 1.38, ns = 40 vs. 7, respectively, t(45) = 1.94, p = .36.

Next, respondents (n = 39) were asked to list problems “encountered in using the FTOA.” The most commonly cited difficulties included lack of compliance by therapists, clients, or supervisors (n = 21, 54%), followed by insufficient data (n = 16, 41%) and cost (n = 6, 15%).

Types of FTOA Measures

Clinic directors were also queried about when FTOA data were collected at their settings and asked to identify the types of instruments used in gathering such data. Results indicated that FTOA assessments were conducted at all respondent clinics prior to treatment (n = 45), during treatment at 76% (n = 34), at termination at 98% (n = 44), and at follow-up by 29% (n = 13). Table 1 lists those assessment procedures and instruments reported used by at least 20% (n = 9 or more clinics) during at least one of these four time intervals.

Discussion

Study findings must be interpreted with some caution in view of the 40% return rate. There can be no assurance that the practices and experiences reported by our sample mirror those of the entire population of clinical psychology training clinics. However, it is hoped that a description of current practices at a large number of training clinics will be of particular interest to those who wish to include such clinics in a national practice research network as well as those who wish to add or improve FTOA collection programs at their clinics. Consequently, the following generalizations are offered tentatively:

1. In a large number of clinical psychology training clinics, at least some supervisors routinely conduct formal evaluations of treatment outcome. Reasons for not conducting FTOAs included lack of consensus among supervisors, limited client resources, and reservations about usefulness.

2. FTOA data are used primarily for research purposes and for feedback to guide training and quality monitoring. However, it is rarely used in formal evaluations of trainees or supervisors.

3. In most clinics, free access to FTOA data is restricted to the case therapist and supervisor along with the clinic director. At no clinics could nonclinical faculty access case material without securing permission, and in only about one quarter of such clinics was permission sometimes granted.

4. While concerns exist, the majority of directors of clinics at which FTOAs are conducted report them to be at least moderately helpful. Benefits frequently noted include aiding in student training, treatment assessment and planning, and quality control.

5. Thirty percent of clinics that use FTOAs for research purposes apparently do not obtain IRB approval for their investigatory activities. This finding was unexpected and raises a question that we believe deserves careful examination: Under what circumstances (if any) can data collection procedures involving therapy clients be legitimately exempted from the protective oversight of an IRB board?

6. A substantial number of directors also noted problems associated with FTOAs. Most frequently cited was difficulty obtaining adequate compliance from therapists, supervisors, and clients. Also cited was insufficient data (e.g., “N too small,” “insufficient follow-up data”), and cost (e.g., required too much time from clients or staff).

7. Most commonly used instruments for conducting FTOAs included target problem ratings and symptom measures such as the BDI-II (Beck, Steer, & Brown, 1996) and SCL-90-R (Derogatis, 1994). The MMPI-II (Hathaway & McKinley, 1989) was frequently used as a pretreatment measure, but not at termination or follow-up.

Of some interest is the fact that our results suggested that a lower percentage of clinics (57%) conduct outcome assessments than was reported in a 1985 survey (68%) by Stevenson and Norcross. A possible explanation for the difference is that our definition of “formal assessment” (see above) may have been more restrictive. Stevenson and Norcross, for example, appear to have counted as outcome assessment procedures “subjective” ratings by clients and therapists. Alternatively, if, in our analyses, the 11 clinics at which some, but not all, supervisors conduct FTOAs are included, the percentage of clinics at which FTOAs are conducted (by at least some supervisors) rises to 72%.

It is also possible that FTOA data gathering is declining in popularity, a finding that could portend additional TABLE 1

<table>
<thead>
<tr>
<th>Measure</th>
<th>Number and (%) of Clinics</th>
</tr>
</thead>
<tbody>
<tr>
<td>Client target problem rating</td>
<td>Pre-tx</td>
</tr>
<tr>
<td>Therapist target problem rating</td>
<td>23 (51)</td>
</tr>
<tr>
<td>MMPI-II</td>
<td>15 (33)</td>
</tr>
<tr>
<td>BDI-II</td>
<td>25 (56)</td>
</tr>
<tr>
<td>STAI</td>
<td>11 (24)</td>
</tr>
<tr>
<td>SCL-90-R/BSI</td>
<td>21 (47)</td>
</tr>
<tr>
<td>CBCL</td>
<td>9 (20)</td>
</tr>
<tr>
<td>OQ45</td>
<td>8 (18)</td>
</tr>
</tbody>
</table>

Note. MMPI-II = Minnesota Multiphasic Personality Inventory-II (Hathaway & McKinley, 1989); BDI-II = Beck Depression Inventory-II (Beck, Steer, & Brown, 1996); STAI = State-Trait Anxiety Inventory (Spielberger, 1983); SCL-90-R = Symptom Checklist 90-Revised (Derogatis, 1994); BSI = Brief Symptom Inventory (Derogatis & Melisaratos, 1983); CBCL = Child Behavior Checklist (Achenbach & Edelbrock, 1985); OQ45 = Outcome Questionnaire 45 (Lambert et al., 1996).
challenges to the inclusion of psychology training clinic data in a national practice research network. Further, such a result would raise concerns about the extent to which psychology training clinics are adequately addressing the challenge of integrating clinical practice with clinical science (Borkovec, 2001; Halgin, 1986; Stevenson & Norcross, 1985; Todd et al., 1992). Whether such a trend away from outcomes assessment (as opposed to increasing the regularity of this practice) is desirable from either an ethical and/or scientist-practitioner perspectives remains an important issue.

References

Convention 2002

Quit While You’re Ahead, Frown When You Play Poker, and Watch Out for the Craps Stick: Gambling Tips for Attendees at AABT’s Upcoming Conference in Reno

Patrick C. Friman, University of Nevada, Reno

In the following array of cities chosen to host AABT’s annual conventions, pick the one that does not belong: Toronto, New York, Los Angeles, Philadelphia, Washington, Chicago, Atlanta, and Reno. If you picked Toronto because it’s in Canada or because of the uniquely high concentration of diphthongs spoken there, your reasoning would be right but your choice would be wrong. If you picked Washington because it is the only one with no voice in a national representative governmental system, your reasoning would again be right, your politics would probably be left, but your choice would also be wrong. The correct answer is Reno! Reno is the only one of these cities in which people cannot see the air they are breathing. Another distinguishing feature

of it now. As a local, a fellow behavior therapist, and a good host, I feel compelled to provide some gambling advice.

To wit: don’t. That’s the best advice I have, but the odds of many people following it are slim. See how insidiously pervasive gambling is in Reno. Here just a little more than 2 years and I now habitually speak in terms of odds, long shots, good bets, dumb luck, and winning streaks. Anyway, I am betting (see what I mean) that many of you, maybe most, are going to ignore my best advice, so you may want to consult published gambling guides or at least consider the advice below.

Quit While You Are Ahead

Odds are against you in every single game except poker, but variations in those odds will periodically put you in the winner’s column, at least for a while, and that is the best time to quit. Unfortunately winning tends to make novice gamblers think they actually know what they are doing, which leads to more playing, which leads to losing. The law of large numbers provides a logarithmic basis for that argument. The thinned wallets and emptied purses of gamblers leaving the casinos provide supportive data. The problem is that the excitement and the glitter of the casino coupled with winning money, getting money for nothing, tends to drown out the little voice in players’ heads that tells them something is not right. What little voice? you may be wondering. Hint: It’s
the voice doing the wondering. Without the guidance the little voice provides, gamblers are driving without brakes, skiing without poles, sailing without a rudder. You get my point. So if the little voice in your head gets drowned out, listen to my voice instead: Send all of your money to the correspondence address for this paper. Actually, you can avoid having your little voice muted in the casino by training it now to forcefully insist that you set a strict limit on what you can lose and quit while you are ahead. Quitting while you are ahead is the only way to win. I'm assuming the logic of this position is sufficiently clear to excuse me from further elaboration. Very few do it, which is all the more reason to mention it now. Here is something else you should know: The easier a game is to play, the more likely you are to lose.

Easy to Play, Hard to Win

Keno

Keno, for example, is really easy to play. In fact, Keno has the distinction of being the only casino game you can play while using the bathroom. Using free crayons, you just X out up to 10 squares numbered from 1 to 80 on Keno sheets and hand the sheet to the nearest Keno person. I say Keno person instead of Keno lady because university hiring policies and search committee politics have trained me to at least allow the possibility that there are Keno men. I have never seen a Keno man, nor do I know anyone who has seen one, but in principle, at least, they exist. Anyway, when the Keno person takes your Keno sheet you place your bet. I recommend the lowest possible bet because science has shown that the size of a Keno bet is inversely proportional to the size of thebettor’s IQ. After all the suckers (Keno enthusiasts) have turned in their sheets, the Keno machine, a device that randomly distributes 80 numbers and then selects 20, is turned on. In order to ensure that players can readily check their sheets to see if they have won, the 20 numbers are sequentially displayed on electronic boards across the casino and also on television, radio, the Internet, at local drive-in theatres, and overhead by a squadron of skywriters. In short, Keno is simple and the casinos make it really easy to play. Winning, however, is as hard as playing is easy. For example, the odds of picking out eight Keno numbers and having all eight selected by the Keno machine has been calculated to be 1 over a number corresponding to all the grains of sand on all the beaches and deserts on earth.

Slot Machines

Slot machines are also really easy to play and consistent with the theme I am developing here, the chances of beating the slots are correspondingly low. With slot machines, though, there is no middleman, no slot machine person, as it were. It’s just you and the machine. You put your money in, and the machine takes it away. If you keep putting money in, the machine will intermittently give you some back, less than you put in, mind you, but some back just the same. This arrangement is the result of behaviorism. Back in the 20th century casinos consulted with all the big behaviorists and they suggested casinos let the losers (slots enthusiasts) win every so often. Apparently the win-every-so-often formula matches the most powerful schedule of reinforcement known to humankind. Not satisfied with this powerful reinforcement scheme, the casinos also consulted with several students of Ivan Pavlov, who encouraged them to have bells ring whenever a slots player won. But instead of salivating, when slots players hear bells, they start asking people if they have change for a 5-dollar bill.

Harder to Play, Easier to Win

Blackjack

Blackjack is a little more complicated than Keno or slots and thus a little easier to win. When I say easier I do not mean easy. Five hundred pounds is easier to lift than 600. A tiny précis may be in order. Numbered cards are assigned their face value (e.g., a 7 is worth 7), face cards are assigned a value of 10, and aces can be used as 1 or 11. Players are dealt two cards each and are then allowed to take as many cards as they want until they meet or exceed 21. Players do not play each other; they play the dealer, who goes last. Closest to 21 wins. Years ago, it was closest to 20, but the casinos found that players were using their fingers and toes to count so they changed it to 21 to make it harder. Players responded by ordering one drink and putting it right behind their cards where they could see (count) it. With the drink plus fingers and toes method, the game became almost as easy as it was when it only involved counting to 20. Some players apparently don’t have all their fingers and toes and thus order extra drinks to make up the difference. At least that’s what I assume as I walk by blackjack tables and see players with several drinks in front of them. I also assume that these players are missing toes, not fingers, because I can usually see their fingers, and . . . they appear to have some difficulty walking.

Craps

Craps is a game of chance that uses dice. An incredibly large number of possible combinations emerge when two dice are thrown, thus statistics are involved. Actually, it’s pretty well known that dice are the primary cause of statistics. Because of all the possibilities and because of the statistics, craps is one of the hardest games to play. A clue about its difficulty is that it takes four persons to run the game (compared with keno or blackjack, which only take one, or slots, which takes none).

Three craps persons are on one side of the table and they watch the board, dice, and money. The fourth craps person is on the other side of the table and he or she watches the players while holding a long craps stick. Watch out for that stick. The craps person uses it to push the dice to the shooter and to whack the hands of players who make mistakes. Anyway, craps is a complex game and, thus, it is easier to win at it than at the simpler games I mentioned above. Not easier, easier. One caveat, of course, is that you have to know how to play it well and I am not going to tell you how because I do not know myself. One look at my bruised hands should tell you that. Actually, I do know one thing. If you want to know the best bet to make, offer to make one for the craps persons. This is tantamount to a tip, only it is wagered on behalf of the craps persons rather than given outright. If the bet wins, the craps persons get the money and they enthusiastically acknowledge your gambling acumen. If the bet loses, the craps person with the stick whacks your hands. How does making a bet for the craps persons provide information? The craps persons have studied every angle of the game and are at the craps table all day, every day. So they know the best bets. But beware of asking them directly what to bet on. That is a mistake, and if you make it, your hands are likely to do some business with the craps stick. Just give them a little money to make a bet for themselves and they will show you the best bet by making it with your money.

Poker

Speaking of good bets, let’s talk poker. It is arguably the most difficult game to play in the casinos and the one with the best chances for the player to win. For a player who knows the game well, playing poker is not even gambling. The reasoning behind this claim is a bit extensive for this little paper so perhaps you and I can play poker together some day and then I can show you what I mean (bring lots of money). I’m not going to elaborate on the rules of poker here but I will give you a little advice.
Before you sit down to play, in fact before you even enter the poker area, you should use imaginal exercises to summon up a somber mood. As you survey the casino scene, note the difference between the frowns at the poker tables and the frivolity everywhere else. The look on the average poker player’s face is somewhere between one you would expect just after a funeral or just before a much needed bowel movement. Poker is serious stuff, and fun, if it is being had, must not be shown. This rule is probably to protect the winners. In poker the players play each other, not the house; thus, when a player wins, the money won comes from the other players and unless the other players are psychotic or drunk, they are unhappy about losing. But why do all the players frown, you may wonder. Most frown because they are losing, and thus, by some pretty straightforward logic, are losers. Those who are even (neither winning nor losing) are frowning because they are bored. And those who are winning are frowning so that the losers won’t beat them up. If the ubiquitous frowning makes it hard for you to identify the winners, here is a hint: Players who have an extra place at the poker table just to hold their chips are usually winners.

Here is a little more advice. If you are a novice poker player, do not play in the no-limit poker game at the El Dorado casino downtown. The El Dorado is a wonderful casino, one of the most popular in town and deservedly so. It also has a great poker room with highly professional dealers, very appealing cocktail persons, and plenty of first-rate second-hand smoke. It is truly worth visiting. But if you have not played high-stakes poker before, do not start there. The difference between no-limit and limit poker is like the difference between lightning and lightning bug, as Mark Twain might have said, or between strip poker and strip search, as I might say. You know the old saying: “If you don’t know who the sucker is, it’s you.” If you were to sit down at the no-limit game at the El Dorado, everyone at the table, including the dealer, would immediately know who the sucker was. Then if you took a moment to remember the old saying, it would be unanimous. The regulars at the no-limit game uphold the frown code and so it’s hard to guess what they are actually thinking and feeling as the novice joins their game. But if the novice had special glasses that provided psychological X rays of others, the regular players would look vulpine with large glistening snouts, huge protruding canines, and copious saliva foaming at the creases of their jaws. And that would just be the women players.

Even professional poker players are wary of no-limit poker. A player can have an incredible winning streak lasting for hours and lose everything on just one hand. In that sense, it’s a little like defending your dissertation proposal. In fact, the similarities are remarkable. The novice player (student) goes into a room where veteran players (faculty) are seated around a table steadfastly adhering to the “no-fun, all-frown code.” After a little chitchat that goes absolutely nowhere, the novice player joins the seasoned players at the table and starts to make bets (claims) that the seasoned players then call (challenge) and raise (ask how the claims pertain to their own work). The seasoned players also do a lot of bluffing (pretending they have read the proposal, know the theory behind the claims, have the student’s best interests at heart, etc.). The outcomes of no-limit poker for novices and of dissertation proposal defenses for students are also very similar. Both player and student usually leave the table with very little of what they had when they sat down.

The Hilton (where the convention is to be held and you will be staying) and several casinos downtown (e.g., El Dorado) have numerous limit games. Limit games are those that limit what you can bet. Most of them are either $1–$4 or $3–$6. The amount on the left of the dash is what you can bet until the last card and the amount on the right is what you can bet on the last card. There will also be the occasional $3–$6 full-kill game. Hidden in the name of that game is a clue about whether I think you should play it. See if you can find the clue.

Even novices can do OK in limit games if they develop two skills: (1) lying with a straight face; (2) detecting lies told with a straight face. There are some other skills involved, but since you might be playing me, I’m keeping them to myself (I’m a good host but I’m not an idiot, despite what the craps persons with the sticks think). Regarding the lying, when I have a weak hand I will often compose my face so that it resembles a cow contemplating the open range and then ask fellow players questions such as, “What does it mean when all the cards are in sequence and in the same suit?” The other critical skill involves detection of lies. More generally, detection of unconsciously emitted behaviors that suggest what a player is really thinking is an ability all good poker players have. Psychologists usually presume to have this skill too. For most psychologists, however, is way more presuming than detecting, and that is why I especially like to play poker with them. These unconsciously emitted behaviors are called “tells” by poker aficionados. For example, if after a player looks at his/her cards and then clasps his/her actual hand to his/her forehead and audibly mumbles, “Holy cow, these are all aces,” that would be a tell.

**Hard to Play, Hard to Win**

I have two other bits of advice pertaining to other casino games of chance. One is to limit your play at exotic-looking games with foreign-sounding names. For some examples, watch a few James Bond movies. Unless you have abundant capital, by the time you learn what is going on in these games, you’ll be out of money. The other is to limit your play at any games that involve winning a car, for two reasons. First, if the odds of winning the game were good, the casino would not have to throw in the chance of a winning a car to get you to play. Second, the odds of winning the car are usually somewhere in the range of a number corresponding to 1 over 1,000 times the number of all the grains of sand on all the beaches and deserts on earth.

**Nothing Is Free**

While you are gambling, cocktail persons (ladies) will bring you alcoholic beverages and not ask for money in return. As with Keno persons, I admit it is possible that there are cocktail men, but I have not seen one. Even though players do not pay for the drinks, they are not free. The payment schedule is merely delayed. With drinks you pay nothing on the front end and way more than the drinks are worth on the back end. How does this work? Well, as you may have learned by now, gambling involves math (e.g., counting to 21) and as you probably learned in high school, alcohol and math do not mix, especially gambling math. Compounding the problem is the effect alcohol has on the little voice in our heads. Researchers working in the addictions have known for years that as drinkers drink, the little voice actually gets louder—although drinkers tend to ignore it because it slurs its words so badly.

There is a really interesting book, *Tipping Point*, by Malcolm Gladwell. Gladwell’s theory is that some seemingly very small events in life can make an enormous difference in the events that follow by changing or tipping them into a new domain. For example, the difference of just one degree Fahrenheit can tip water from a solid to a liquid or from a liquid to a vapor. There are also developmental tipping points. In the progression of routine developmental events in a child’s life, reaching certain milestones tips the child into entirely new domains. Progressing from crawling to walking, babbling to...
speaking, and asking for car keys instead of a ride are developmental tipping points. Inspired by Gladwell’s theory, I developed a theory of my own called the Tipping Over Point. It pertains to drinking. In a drinking episode, there is ultimately one drink the ingestion of which tips the drinker into a new domain because the drinker tips over. Then the drinker develops backwards, regressing from walking to crawling, from speaking to babbling, and from driving to surrendering car keys to the nice officer and asking for a ride home—usually provided after a short visit to the local police station to complete some paperwork.

Then, of course, there is just plain tipping. A student who reviewed a draft of this paper was formerly a cocktail person in a casino. She suggested that I strongly recommend tipping. In fact, she was a little more emphatic than that. She said that when casino patrons do not tip cocktail persons for services, the patrons deserve to be tipped over. So my advice for novice gamblers is to avoid drinking. But if that advice is to be ignored, then I advise you drinkers to drink sparingly, so as to avoid the tipping over point, and to tip generously, so as to avoid being tipped over by a miffed cocktail person.

Best Bets

Although the games of chance in Reno are hard to beat, it is truly possible to leave the upcoming annual convention a winner. For openers, there is the convention itself, always a good bet. The Hilton is a great hotel with deluxe accommodations considerably more affordable than when the convention is held in more conventional convention cities (remember my first paragraph?). The Hilton is also a terrific place to hang out. As just one example, gambling is a very interesting spectator sport and the Hilton is ideally set up for prime people watching. Throw in the Axis II level of voyeurism in most psychologists and you have a surefire formula for having fun on the cheap. Then there is Reno itself. Reno provides a blend of man (person) made and nature-made wonders found in few other places in this country.

SUBSTANCE ABUSE TREATMENT AND HEALTH SERVICE RESEARCH: PROJECT DIRECTOR AND POSTDOCTORAL FELLOW. The National Center of Addiction and Substance Abuse at Columbia University (CASA) has openings for a Project Director and a Postdoctoral Fellow to assist in its treatment and health services research program. CASA is a unique think/action tank that brings together under one roof all the professional disciplines (health policy, medicine, nursing, communications, economics, sociology, anthropology, law, law enforcement, business, education, policy) needed to study and combat all forms of substance abuse—illegal drugs, pills, alcohol and tobacco—as they affect all aspects of society. The Project Director will assist with a newly funded project examining the effectiveness of several interventions to treat substance abusers on welfare in New York City. The Project Director will assist senior investigators with issues related to study design, collection of outcome data, data management, data analysis, and preparation of scholarly publications. Duties will also include overseeing the day-to-day data collection of the research project, training and supervising research assistants, and ensuring data integrity. Applicants should possess a doctoral degree in the social sciences and possess excellent computer, managerial, and data analytic skills. Experience in substance abuse treatment and cost-effectiveness research is highly desirable.

By night it has a spectacularly crenellated skyline that is more suggestive of a city with a 1 million–plus population than of a city with a mere 300,000. By day there are mountains that ring the city, skies that are blue 300 or more days a year, and temperatures that are moderate even in November. Thirty miles in one direction is Lake Tahoe and the world-class ski areas that surround it. Thirty miles in another direction is Pyramid Lake, one of the most eerily mystical bodies of water in the West. Running through the city of Reno is the crystal-clear Truckee River, famous for its trout fishing. It’s easy to win the game of chance called life in a place like this. My hope is that you will come to AABT in the “biggest little city in the world” nestled a mile high up in the Sierra Nevada mountains. My advice is to gamble with great care if you gamble at all. And my bet is that after your short stay, the wisdom of choosing Reno for the annual convention will be clear. I’d write more on this topic but my little voice is chiding me to quit while I’m ahead.
and grant writing in the areas of substance abuse treatment and health services research. Some clinical experience may be provided if needed for licensure. The goal of this postdoctoral fellowship is to provide the skills needed for postdoctoral researchers. The position offers competitive salary and excellent benefits.

Interested applicants should send a cover letter explaining their interest, a curriculum vitae, and three letters of reference, and salary requirements to: Dr. Jon Morgenstern, Vice President, Health Treatment and Research Division at the National Center on Addiction and Substance Abuse at Columbia University, 633 Third Avenue, 19th Floor, New York, NY 10017 or via e-mail to jmorgens@casacolumbia.org. E/O/E

FULL-TIME CLINICAL PSYCHOLOGIST. The American Institute for Cognitive Therapy seeks a licensed psychologist with the highest level of proficiency in cognitive-behavioral therapy. Individuals interested in this staff position that begins October 2002 will see a full case load for individual psychotherapy, conduct supervision, market our practice, and contribute to ongoing research at the Institute. We are recognized as a leading treatment and training center. Qualified candidates should send a letter of interest, vita, and 3 letters of recommendation to Randye Semple at: American Institute for Cognitive Therapy, 136 East 57th Street, Suite 1101, New York, NY 10022. http://www.CognitiveTherapyNYC.com

ASSISTANT PROFESSOR. School of Professional Psychology, Pacific University, Forest Grove, Oregon: This is a tenure track position in a pleasant academic environment for an ambitious child behavior therapist who has strong research, clinical, and teaching skills. The School of Professional Psychology has an APA accredited doctoral clinical psychology program, a nationally accredited Masters in Counseling Psychology program, its own large Psychological Service Center in downtown Portland, and an APPIC approved internship training program. The position involves teaching in the Masters and Doctoral programs (the equivalent of six courses per year), supervision of masters' theses and doctoral dissertations, and clinical supervision of students. The applicant must be licensed in Oregon or eligible for Oregon licensure. Pacific University is an Equal Opportunity Employer. Please mail a cover letter, a curriculum vita, and three letters of recommendation to Paula Truax, Ph.D., Pacific University, 511 SW 10th, #400, Portland, OR 97205.

CLINICAL PSYCHOLOGIST. Dean Medical Center, a 400+ physician clinic, is searching for a full-time doctoral level clinical psychologist with cognitive-behavioral expertise to join our 40+ member, multidisciplinary psychiatry department. The position will be at our clinic located in central Madison. Applicants must have Ph.D. or Psy.D. in clinical psychology from an APA-accredited doctoral program, and APA-accredited internship, and be eligible for licensure in Wisconsin and for inclusion in the National Register. The successful candidate will have documented specialization in cognitive-behavioral therapy and be skilled in working with a broad age range of clients. Strong preference given to those with minimum two years postdoctoral experience. Solid diagnostic skills, collaborative style and excellent communication skills essential. Experience within medical setting a plus. Responsibilities include: psychodiagnostic evaluation; individual, couple and group treatment modalities; psychological testing; staff supervision and consultation to medical staff; limited call and inpatient work. Carry a full case load of patients. Excellent salary and benefits. Send resume and letter of introduction to: Emily R. Hauck, Ph.D., Dept. of Psychiatry, Dean Medical Center, 1313 Fish Hatchery Rd., Madison, WI 53715

CHILD PSYCHOLOGIST—MADISON, WISCONSIN. Dean Medical Center, a 455 physician medical center, has an opening for a full time doctoral level child clinical psychologist to join a 41-member multidisciplinary psychiatry department serving a wide range of patients. The primary work site, located on the west side of Madison, WI, is at the medical center's satellite clinic, which includes a 10-member psychiatry department team. Responsibilities include: psychodiagnostic evaluation of children (emphasis on ages 12 and under) including interviewing of children, parents and collateral professionals, and psychological testing; treatment planning; individual, family and group psychotherapy with outpatient children; some limited inpatient work; consultation with medical staff and schools; supervision of masters-level staff within the department; and limited call.

Applicants must have Ph.D. or Psy.D. in clinical psychology form an APA-accredited doctoral program with documented specialization and academic preparation in child assessment and treatment, an APA-accredited internship, and must be eligible for licensure in Wisconsin and for inclusion in the National Register. Thorough assessment skills, including a testing background with young children and evaluation of child psychopathology and family systems issues, are essential. Must have strong therapeutic skills; be adept at working with parents and families as well as children; be able to do focused, time limited work as well as intensive, long term treatment; and have solid teaming skills with other professionals (physicians, teachers). Preference given to those with experience and strong interest working at the interface of medicine and psychology. Excellent salary and benefits. Send resume and letter of introduction to: Nancy L. Gerz, Psy.D., Dept. of Psychiatry, Dean West Clinic, 752 N. High Point Rd., Madison, WI 53717.

HUDSON RIVER REGIONAL PREDOC TORAL INTERNSHIP PROGRAM IN PROFESSIONAL PSYCHOLOGY. NEW YORK STATE OFFICE OF MENTAL HEALTH offers full time predoctoral internship positions in professional psychology for 2003-2004 in its APA-accredited program. Weekly seminars in a variety of clinical and professional areas supplement extensive supervision. Clinical assignments are to inpatient and community services programs at facilities of the New York State Office of Mental Health. Preference is given to students enrolled in APA-accredited clinical or counseling psychology programs. For further information and application materials, write to Paul Margolies, Ph.D., Training Director, Hudson River Regional Psychology Internship Program, Hudson River Psychiatric Center, 10 Ros Circle, Poughkeepsie, New York 12601-1078. You may e-mail your request to www.hrrhipm@omh.state.ny.us.

SCHOOL PSYCHOLOGIST: Ph.D. Program in Educational Psychology, The Graduate Center, The City University of New York. We anticipate a tenure-track position in our APA-accredited school psychology specialization for either the Spring 2003 or the Fall 2003 semester (rank open). Teaching responsibilities include two doctoral courses per semester and doctoral advisement in our cognitive behavioral school psychology specialization. The candidate is expected to demonstrate strong research history or research potential. The candidate must be eligible for license eligible in New York State. A research and training background in neuropsychology is desirable. Review of applications will begin October 1, 2002, and continue until the position is filled. Send letter of application, current vita, official transcript, and three letters of reference to: G.S. Tryon, Ph.D., Ph.D. Program in Educational Psychology, CUNY Graduate Center, 365 Fifth Avenue, New York, NY 10016. For additional information contact G. Tryon via e-mail at gtryon@gc.cuny.edu EO/AA/IRCA/ADA Employer.

CLINICAL PSYCHOLOGIST, SUNY STONY BROOK. The Psychology Department invites applications for a tenure track position at the Assistant Professor level, although outstanding applicants at the Associate level will be considered. The position begins Fall 2003, pending budgetary approval. Applicants must have a record of first-rate research and be prepared to provide high-quality teaching within undergraduate and graduate programs. Outstanding clinical scientists in all areas of Clinical Psychology are encouraged to apply, with a preference for candidates who study either basic processes in psychopathology, with focus on one of the major disorders, or for intervention for clinical problems. Stony Brook offers an unusually supportive research environment and competitive salaries. Deadline for application is October 1, 2002, or until position is filled. A vita, selected reprints/preprints, and at least three letters of reference should be sent to: Chair, Clinical Search Committee, Department of Psychology, SUNY, Stony Brook, NY 11794-2500. Information about the department can be obtained at http://www.psychology.sunysb.edu. AA/EEO.

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September 2002
After running from session to session, how do I relax at the Convention?

**Networking Lunch**. Sign up for the Friday lunch to begin meeting colleagues with similar interests and concerns. For only $15, avoid long lines at food outlets, have a great lunch, and establish some new connections. See pages xvi and xvii in the Program Book.

**SIG Exposition & Opening Night Cocktail Party**. EVERYONE goes to this event. It is a sure way to run into people with whom you attended grad school, served internship, or had a drink with at the Philadelphia Convention. And, you get to look over the work of members of all the Special Interest Groups.

**Poster Sessions**. With almost 1,000 posters being presented this year, it will be almost impossible to spend time at every board. So before the Convention go through and highlight those posters that you absolutely must see. And read through all the sessions because many times population, problem area, or methodology might place a poster in an unexpected group.

**Bowling for Scholars**. The Reno Hilton is home to a 50-lane Bowling Center. How could we pass up this great opportunity to see "raw" competition at its best? See page xix in the Program Book for registration info. Cheerleaders and hecklers will be welcome. This has got to be an amazing experience.

**Saturday Night DJ Party**. After the Bowling, or a great dinner, come dance at the Annual Saturday Night Party. A great way to unwind.